are trying to explain. Explanation should be the most important objective of the scholarly enterprise, not simple description of a phenomenon. Eclectic approaches, if not used carefully, can end up in thick descriptions of a mish-mash variety, a danger that scholars should try to avoid. Also, sometimes a single paradigm can explain a given phenomenon better and if so, one should be willing to accept that possibility without branding himself as a card-carrying member of a paradigm. Sil and Katzenstein offer us a coherent set of philosophical foundations and a flexible analytic framework for research that does not fit into existing paradigms. Now, it is up to other scholars to do more of the problem-focused empirical work needed to make this a core approach in the discipline.

Analytic Eclecticism: Not Perfect, but Indispensable

https://doi.org/10.5281/zenodo.936293

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Following the pragmatist bent of our conceptualization of analytic eclecticism, we view the four excellent commentaries offered above as a welcome opportunity to engage in open-minded dialogue and to clarify certain defining attributes of eclectic scholarship. Two commentaries are offered by Andrew Bennett and Peter Haas, who are sympathetic critics of our work: They accept our premise that research confined to paradigms has built-in limits, but then offer some frank assessments of the challenges and limitations of analytic eclecticism. They raise important issues about the implications of analytic eclecticism for the cumulation and assessment of theories, and for some of the risks that existing institutional practices pose to eclectic scholarship. We regard their remarks less as a critique of our arguments and more as impressive forays into the sorts of discussions that we hope will displace stale inter-paradigm debates if academic scholarship is to become more connected to the world of policy and practice (Calhoun 2009; Nye 2009; Shapiro 2005). The other two commentaries, by Alice Ba and T.V. Paul, come from "insiders" in the sense that both are authors of works we discuss in our book (2010b) as exemplars of eclectic scholarship. Admittedly, they have little incentive to critique a book that casts their work in a favorable light. Their thoughtful and candid reflections, however, go a long way toward helping us clarify some important points about the different pathways to analytic eclecticism and the complexities involved in coding what counts as eclectic scholarship.

Let us begin by making clear what these scholars are commenting on: Bennett and Haas are addressing an article published in *Perspectives on Politics* (2010a) as well as our book, *Beyond Paradigms* (2010b); Ba and Paul are primarily reacting to the latter. The article is a general programmatic statement, outlining the intellectual rationale and philosophical foundations for eclectic work in both comparative politics and international relations. It develops a consistent set of markers for distinguishing between scholarship embedded in research traditions and analytically eclectic research. In addition, it underscores eclecticism's reliance on an expansive and open-ended definition of causal mechanisms, at least for analyzing problems that feature "more extensive endogeneity and the ubiquity of complex interaction effects" (Hall 2003). The book offers the same general argument, but focuses solely on international relations and employs the term "paradigm," which is more commonly used to characterize contending schools of thought in that field. The book is not an anthology but offers instead a sustained, coherent argument. We did not ask for permission to label as "analytically eclectic" any of the fifteen works discussed—five in each of three substantive chapters discussing eclectic research in the analysis of security, political economy and global governance. Nor did we extract any passages or selections from any of these studies. Instead, we deployed our own criteria for deciding which studies constitute reasonable approximations of analytic eclecticism in international relations (2010b: 19–23): open-ended problem formulation, a complex causal story featuring mechanisms from multiple paradigms, and pragmatic engagement with issues of policy and practice. We then discuss the content of several exemplary studies in a manner that highlights their eclectic character, their distinctiveness vis-à-vis paradigm-bound research on the relevant topic, and their value-added within the context of scholarly efforts to understand particular aspects of international life. Finally, we incorporate brief 400-word statements (presented in fifteen boxes in the book) from each of the scholars, each responding to specific questions we posed about their experiences in producing eclectic scholarship.

Bennett's essay is running a bit ahead of us. It pushes us after the publication of our book to confront more fully issues that we see as arising if there were to follow a wider acceptance of the assumptions and practices associated with analytic eclecticism. Because of the prevalence of a paradigm-centered view of international relations scholarship, we were primarily focused on clarifying the trade-offs between work embedded in paradigms or research traditions on the one hand and eclectic research on the other. We also sought to highlight the advantages of eclectic scholarship for addressing specific problems in world politics. Since, to date, there does not yet exist a critical mass of eclectic research on any given problem, we touched only briefly on the questions Bennett wishes us to now pursue. We welcome the implied optimism about the potential impact of our formulation of analytic eclecticism on scholarly practices. And we see his articulate commentary as an invitation to reflect further upon the implications of adopting an eclectic approach for the character, utility, and evolution of social scientific knowledge.

First, it is worth noting that our ambitions are more limited than Bennett's. We are gratified that Bennett notes our tracking of current trends in the philosophy of science. It is, however, worth emphasizing that we proceed from foundations that are more closely associated with neopragmatism rather than with versions of philosophical realism or neopositivism. Certain strands of realism and pragmatism do have much in common, but pragmatism is more explicit about several points we consider crucial for defending analytic eclecticism: it calls for greater scope for deliberation among a more inclusive community of inquirers; it discounts the separation between abstract knowledge and practical insights relevant to a specific situation; and it views all knowledge as a set of tentative insights that can never be confirmed as "*the* truth" but can be updated and recombined in different ways in concrete situations (Sil and Katzenstein 2010b: 43–48). Furthermore, analytic eclecticism does not aspire to general theories that cover different types of problems. Rather it is restricted to the level of "middle range" theory as identified by Robert Merton's understanding of the term. This means that theoretical constructs—concepts and propositions—are designed to gain traction on a set of similar problems clearly demarcated and easily identified in concrete situations that recur in world politics. An eclectic approach that is grounded in pragmatism and is limited in aspiration to middle-range theorizing does not aim at ambitious, holistic understandings of "theory cumulation" across entire disciplines or subfields. And it does not facilitate a direct assessment and comparison of theories that deal with different problems even if these are loosely categorized under similar labels such as "security" or "political economy."

At the same time, as Bennett recognizes, the combinatorial forms of knowledge we associate with analytic eclecticism can be organized in such a way that they allow us to identify discrete configurations of causal mechanisms that are portable across those contexts within which a given problem is identified. Within these limited contexts theory cumulation is possible insofar as there is a progressively deeper understanding of how general mechanisms interact in different environments to generate different or recurrent processes. It is also possible to develop and compare different kinds of analytically eclectic arguments, each proposing distinctive configurations of mechanisms in relation to similar kinds of problems. We have not done this in the book, as we considered instead an array of problems on which eclectic scholarship has provided fresh insights in comparison with arguments developed within paradigms or research traditions. However, once a critical mass of eclectic scholarship has emerged in relation to any given problem, the next step would indeed be to compare eclectic middle-range theories in terms of how plausible the interconnections between general mechanisms are, and how consistently the combined effects of a particular configuration of mechanisms are evident in a given context or environment. An adequate response to what Bennett is asking for would require a companion volume once it becomes easier to locate multiple eclectic approaches to a given problem. For the time being, we are fully cognizant of both the limits and possibilities for theory-cumulation and theory-comparison at the level of mid-range theorizing. We view both as valuable endeavors, but only within a more concretely delimited intellectual terrain, bounded not by disciplines or fields of study but by the attributes of similar concrete problems with similar scope conditions.

Bennett also asks us to consider whether we can assess the long-term value of eclectic scholarship through the lens of a Lakatosian conception of scientific progress (Bennett 2003; Elman and Elman 2003). Such a conception would be based on the identification of "novel facts"—the value of which is established through "use novelty" and "background theory novelty"—and is not incompatible with eclectic modes of scholarly inquiry. Bennett is absolutely right in noting that there is no need to establish paradigms or designate "hard cores" and "outer belts" in order for scholars to arrive at a consensus that specified evidence would have use novelty or background theory novelty relative to specific theories or explanations. However, one aspect of our argument is critical to bear in mind here: Scholars do have a great deal of leeway in how they frame their core problems. Problems can be posed in such a manner that they not only encourage the use of certain methods (Sil 2004), but also draw greater attention to particular mechanisms in particular domains or levels of social reality. Extending Ian Shapiro's (2005: 184) observation about the formulation of research questions in the discipline, we argue:

> The issue, in our view, is not whether the social sciences ought to be problem-driven or method-driven but rather how problems are identified and formulated. Projects embedded in different research traditions frequently address similar or related substantive issues but parse these issues in order to focus on specific aspects in keeping with their theoretical priors. Such simplification of social reality is certainly understandable, even necessary. However, the *extent* to which and the *manner* in which large parts of that reality are simplified in the formulation of problems matter for the purpose of generating insights that bear on the choices and actions of actors coping with complex substantive problems. A pragmatist conception of analytic eclecticism invites us to consider how the problems as defined within research traditions might (or might not) relate to each other and to concrete dilemmas related to policy and practice. (Sil and Katzenstein 2010a: 418–19)

This suggests that the designation of "novel facts" is not a straightforward process when paradigm-bound scholarship is juxtaposed with eclectic arguments about similar problems. For example, "use novelty" would require drawing upon new observations that can serve as additional evidence to support a claim initially based on some other evidence. However, eclectic scholarship often generates claims that are significantly more complex for the simple reason that the scope of the problem is expanded so as to partially reverse the simplifications made by paradigm-bound researchers in posing their questions. Similarly, background theory novelty is a useful standard when a proposition purports to explain anomalies or unexplained phenomena; but what constitutes an anomaly or an unexplained phenomenon is often shaped by the substantive focus and scope conditions inherent in a research question as posed by individual scholars. Since eclectic scholarship is often aimed at problems that incorporate or subsume the more narrowly framed questions taken on by adherents of paradigms, the designation of what constitutes a "novel fact" becomes more problematic than would be the case with theories that take on similar questions formulated within a paradigm.

We do not wish to intimate that Bennett's suggestion is not useful or that theory incommensurability is an intractable problem. In fact, we go to some lengths to establish that the problem of incommensurability is often over-emphasized and that it can be mitigated by translating concepts and analytic principles upon consideration of the empirical referents used to operationalize these (Sil and Katzenstein 2010a: 414–15; 2010b: 13–16). This effectively suggests that applying the Lakatosian standard of novel facts involves a two-step process. The first involves the difficult task of translation and comparison of related problems that have been cast at different levels of abstraction, are formulated in different theoretical vocabularies, and have different scope conditions. Only after this first step has been taken can we be assured that we are discussing a set of theories (whether paradigm-bound or analytically eclectic) that are actually addressing the same problem. And only then can we have a meaningful discussion of whether a given eclectic approach has "use novelty" or "background condition novelty." But before we can tackle intricate problems in the philosophy of the social sciences, the most immediate need is a critical mass of eclectic scholarship that is organized around the complex, messy problems of real-world politics rather than around intellectual puzzles designed to test or apply the concepts and analytic principles associated with existing paradigms.

Like Bennett, Haas also wants to push us forward, to engage questions that we would consider to be significant only if and when analytic eclecticism becomes more widely practiced in the discipline. In the section focused on the philosophy of science, Haas appreciates the utility of eclectic perspectives that train their sights on the interplay of diverse mechanisms. But he insists (rightly) on asking what comes next. In particular, he wonders if we are holding back too much, perhaps constrained by a general commitment to Larry Laudan's (1977, 1996) philosophy of science. Haas is right to note some of the limitations of Laudan's work. Since our interest in Laudan is mediated by a more fundamental commitment to pragmatist philosophy, we are not wedded to every aspect of Laudan's philosophy of science. Our reading of pragmatism (2010a: 416–418; 2010b: 43–48) provides the bridge that Haas is looking for—connecting our commitment to a rigorous social science with our conviction that there is much to be gained from an "assemblage of mid-level findings" organized around concrete problems that have both scholarly and practical import.

We take from Laudan the concept of a research tradition. It offers a more realistic framing of the shifting controversies in the field than the more rigid conceptions of scientific progress offered by Kuhn or Lakatos. Operationalizing a research tradition rigorously is difficult, as Haas points out. But this is even more true of paradigms and research programmes which, as we note in the book, are concepts that do not map on to the messy intellectual history of social science disciplines and subfields. In social scientific research, boundaries are unavoidably fuzzy and encompass more diversity in ontologies and epistemic principles than is commonly recognized. This is why we opt to understand controversies in the social sciences through the lens of Laudan's "research traditions," which eschews a stylized rendering of scientific progress and allows for scholars working in multiple traditions. In the book, we use the term "paradigm" for the sake of simplicity, but we define it as interchangeable with "research tradition."

We also borrow from Laudan the notion that the usefulness of a theory depends more on its ability to solve problems than on its contribution to the cumulation of knowledge. Indeed, a pragmatist perspective sees the very idea of knowledge cumulation as more of an abstract ideal than an ongoing process to which all research inevitably contributes. There is no presumption that eclectic scholarship will gradually pave the way toward all-encompassing "grand truths" that will become firm guides to action and policymaking. Nor would we characterize the results of eclectic, problem-centered mid-level analysis as "small insights" relative to the "broader insights" supposedly generated by more ambitious theoretical claims. In fact, it is precisely grandiose ambitions for unified theories and knowledge cumulation that, in combination with the availability of material and organizational resources, have given rise to the inter-paradigm debates that remain prevalent today.

Moreover, the most self-conscious and self-confident adherents of a paradigm treat knowledge cumulation in an even more restrictive manner. They see cumulation as predicated on acceptance of the metatheoretical assumptions and theoretical principles associated with their preferred paradigm. Yet, the only people who might concur with such a view are those working in the same paradigmatic tradition! Thus, what constitutes "progress" or "cumulation" for some does not constitute progress or cumulation for the field as a whole. Given this predicament, the "small insights" of eclectic scholarship are actually quite "big" in terms of their potential impact, both on the relevant scholarly fields and on policy-relevant debates and discussions. Mid-range eclectic analyses may be modest compared with general laws or grand theories, but we see this modesty as purposeful for the valiant attempts at mediating between different paradigms and connecting academic theories to public discourse and policy debates. For these reasons we do not regard eclectic mid-range theorizing as a way station to a grand theory around which a discipline or subfield will eventually reorganize. Rather, eclectic work, alongside paradigm-bound research, continually accommodates and encourages efforts that aim at translation, comparison, and dialogue within and beyond academic circles.

Haas' remarks about the sociology of knowledge present a more immediate and direct challenge to analytic eclecticism, or at least to scholars considering eclectic approaches. Haas is of course correct that the structures of the discipline militate against the eclectic scholarship we advocate; that was the reason why we wrote the book in the first place. Beyond that, Haas raises the important issue of collaborative work. Haas notes that we rely on single-author works to make the case for eclectic scholarship that is useful, compelling, and rigorous. This overlooks the fact that one of the studies we discuss is actually coauthored (Barnett and Finnemore 2004) and that our book is itself an exercise in collaboration. We thus see no inherent value in single-author eclectic work. Intellectual and practical reasons may push two or more eclectically oriented scholars to collaborate, as we ourselves have. Faced with the intellectual requirements and professional risks associated with eclectic work, having a coauthor or contributing to an anthology may have intellectual, psychological, and professional advantages.

In the contexts of hiring, grant competitions, and promotion, collaborative work of all stripes faces special hurdles in many (but not all) fields and in many (but not all) major research universities. Whatever their intellectual orientation, younger scholars in particular must demonstrate their ability to work independently in designing and executing research projects. Although scholarly research does not need be tied to single individuals, in real life, jobs, tenure reviews, and promotions focus largely on the track record of individuals. This is true in general and does not constitute a specific barrier against the recognition of eclectically oriented work. Whether the social sciences stand to benefit by encouraging and rewarding collaborative research is an important question, but it is one that needs to be addressed on its own terms.

On the difficulties of publishing eclectic scholarship in academic journals, we do not have any fixed prescriptions. New journals, as Haas points out, may be helpful in opening up space for eclectic research that existing journals may shy away from. But the low visibility of new journals means that articles published in them generally are overlooked and are not given the credit they may deserve when the time comes to assess an individual's scholarly productivity and professional standing. The editorial boards and reviewers of well-established journals do learn of course from intellectual currents in the field, but we know little about such learning processes in different parts of the discipline. In the end, if some journals and their editorial boards are clearly partial to specific approaches or methodologies, scholars will have to accept that fact and move on with their lives. At the same time, good social science journals do have to compete with each other for readers and so can ill afford to ignore shifting currents in the field. This means that they have an incentive to occasionally stretch their boundaries to accommodate articles that do not fit their "normal" profile. In these instances, eclectic scholarship may well prove to be a stronger candidate for publication in a journal that favors one paradigm, at least compared with scholarship embedded in a rival paradigm.[1]

Finally, it is important to underline the encouraging fact that we were able to identify eclectic analyses that *have* been published by first-tier journals and top university presses. Many scholars, both senior and junior, are moving away from paradigm-bound research in their own work even as they continue to view the field of international relations as still dominated by contending paradigms. As we observe in the book (2010b: 25), in the 2008 TRIP survey (Jordan et al. 2009: 9, 33), 36 percent of the American respondents (and about the same percentage of the sum total of all respondents from nine other countries combined) indicated that their own work did not fall within one of the major international relations paradigms. This figure constitutes a significant minority; and the percentage is noticeably larger than in previous iterations of the same survey (e.g., Maliniak et al., 2007). Leaving aside the studies we discuss in our book, in several fields scholars are increasingly gravitating towards more eclectic styles of work, although not always self-consciously so. For example, Haas discusses how empirical research in the field of international environmental law has moved away from debates between the "transformational school" and the "enforcement school," and has converged around a provisional consensus on the importance of complex processes that incorporate both inducement mechanisms and learning mechanisms.

The commentaries from Alice Ba and T.V. Paul offer us an opportunity to clarify several important points about the production and identification of analytic eclecticism. First, analytic eclecticism has no set recipe and is not an end in itself. There are many different ways through which different scholars ended up generating work that we code as eclectic. In Ba's case there was no explicit commitment to an eclectic mode of inquiry. Instead her emphasis on *process* pushed her increasingly to look at complex configurations of factors that did not neatly fall within the boundaries of a single paradigm. In Paul's case the movement towards eclecticism cut across several research projects. It began with a limited effort to add nuance to core realist arguments by adding elements from neoliberalism in his earlier book (Paul 2000). In subsequent work Paul (2009) made a more explicit effort to merge ideational aspects normally associated with constructivism with materialist factors emphasized in neorealism and neoliberalism. These observations, as well as the reflections of the other thirteen authors whose work we engage in the book, provided an unexpected bonus in helping us see more clearly and state more forcefully that analytic eclecticism "is not meant to constitute a discrete new 'ism' to replace or subsume all other 'isms' in the field of international relations. It is, however, a useful heuristic for capturing the common requirements of metatheoretical flexibility and theoretical multilingualism necessary for substantive analyses that are not embedded in any one paradigm" (2010b: 25). Ultimately, what makes an eclectic research strategy worth pursuing is the desire to better understand complex, socially important real-world problems that existing paradigm-bound theories either fail to address or address only in part.

In different ways, Ba and Paul also point to the difficulties of promoting an eclectic approach in a field accustomed to viewing scholarship through the lens of inter-paradigm battles. For both, the most unexpected response to their books was not unanticipated criticisms of substantive claims but rather efforts to label the studies using familiar categories. Paul refers to the "pigeonholing" of his work, while Ba refers to her fears that the complex processes she lays out would not satisfy a discipline wedded to the idea of contending paradigms. Old (paradigm-focused) habits die hard. We are not surprised by this reaction. It was a major reason why we wrote our book. We hope that analytic eclecticism and the pluralist spirit it embraces will become a more established part of our disciplinary lexicon, so that authors can focus on the merits of their own work without being concerned about being pigeonholed or about having to manage the expectations of scholars adhering to competing research traditions.

This is a good place to note a particularly vexing problem we encountered when identifying eclectic scholarship: Some authors characterized their books as refined versions of constructivism. This is true of Ba (2009) as well as Martha Finnemore (2003) and Nicolas Jabko (2006). Jabko, for example, characterizes the approach in his book as "strategic constructivism." This pattern is also evident in more general metatheoretical perspectives on international affairs such as the "pragmatic constructivism" articulated by Peter Haas and Ernst Haas (2009). We address this problem directly in our book (2010b: 41–43), emphasizing the need to distinguish between programmatic commitments to constructivism as a distinct paradigm and "weak" identification with constructivism for tactical reasons. The latter, we view as an artifact of the timing and intellectual environment within which constructivism emerged: it was the third of the three major IR paradigms to arrive on the scene, at a time when some neorealists and neoliberals were gravitating towards a rationalist "neo-neo synthesis" (Waever 1996: 163; see also Keohane 1989: 165). Thus, for those wishing to incorporate ideational factors in their analyses, a reasonable path to take has been to distinguish themselves from purely rationalist analyses and to identify themselves with constructivism even if ideational constructs are not actually privileged in ontological or theoretical terms. For such scholars, we hope, analytic eclecticism may present a more appealing alternative to having to squeeze uncomfortably into one of the existing paradigms.

The real issue is the quality and value-added of the individual eclectic approaches. Ba and Paul both underline another caveat that we repeatedly make in both our article (2010a) and book (2010b). Analytic eclecticism does not offer a carte blanche to produce either idiosyncratic stories for each and every case or a never-ending laundry list of factors that potentially influence each and every outcome. Ba is wary of "adhocery" that might trade away standards of good research in exchange for an "everything goes" approach. Paul similarly warns that eclectic analysis should be wary of falling into the trap of providing "thick descriptions of a mish-mash variety." Instead analytic eclecticism gives researchers a license to cut across or operate in between the boundaries separating research traditions. The research contributed by adherents of paradigms identifies important causal mechanisms and makes significant contributions that deserve our serious attention. A commitment to analytic eclecticism does not provide a warrant for ignoring these contributions but rather, as Bennett suggests above, invites us to figure out how different types of mechanisms normally explored in isolation from one another might interact as part of more complex configurations in a given context. As Ba notes in her contribution, our conceptualization of analytic eclecticism is a call "not just for a pragmatic engagement of our empirical problems at hand, but also a pragmatic engagement of existing paradigms as a way to produce more focused, as opposed to scattered, analysis."

We conclude by addressing Bennett's qualms about our choice of the term "analytic eclecticism" in lieu of an alternative that would carry fewer negative connotations. We are surprised that for Bennett the term has negative connotations; the initial spontaneous and unprompted responses to our book and article, from both close colleagues and scholars we do not know, suggest otherwise. In any case, there were many reasons why we chose this term. Some are accidental. Sil had always been struck by the use of the term "eclectic" in a well-known symposium on theory in comparative politics (*World Politics* 1995) in which Katzenstein, Peter Evans, Atul Kohli, James Scott and others highlighted the limits of simplifications in the name of parsimony and acknowledged the "eclectic messy center" at the heart of comparative politics. At the time, however, there had been no serious effort to define "eclectic" scholarship or to explicate the rationale for it. In subsequent work, both Sil and Katzenstein independently developed distinct but complementary understandings of what eclecticism entailed and what it could contribute to, respectively, comparative politics and Asian regional security (Sil 2000, Katzenstein and Okawara 2001/02). When we decided to join forces at a conference in 2003, it made sense to stick to a term that both of us were already comfortable with and that we both were beginning to get identified with in different circles.

Over time we became more self-conscious in embracing the term "eclecticism." Gunther Hellmann (2003: 149) has pointed out that the field of international relations has a particularly strong penchant for "stigmatizing as eclectic whatever approach to current problems in international politics does not fit along the established axes of scholarly enlightenment." This view is also echoed in the statement from Timothy Sinclair that Alice Ba cites in the epithet for her contribution to this symposium: eclecticism has been presumed to be "the ultimate taboo," especially in graduate training. We think that this phase in the evolution of international relations scholarship is passing and that the time has come to turn a fresh page. In our work we thus emphasize the value-added, in both intellectual and practical terms, of eclectic styles of inquiry. It is not our intention to dismiss the scholarship that has emerged from paradigms or traditions. We are, however, convinced that any field that continues for too long to define, pursue, and evaluate research solely through the lens of paradigmatic assumptions and inter-paradigm contests risks missing out on crucial insights about the complex processes and intersecting mechanisms that account for interesting outcomes in world politics. Analytic eclecticism offers a promising way forward.

Note

[1] For example, while it is true, as Haas notes, that *The Journal of Theoretical Politics* was created to publish work on rational choice and game theory, this is the same journal that published Sil's (2000) article on "The Foundations of Eclecticism."

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Symposium: Causal Mechanisms, Process Tracing, and Causal Inference

A Tale of Two Cultures: Causal Mechanisms and Process Tracing

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Our intuitive understandings of causality include a generative process in which a cause yields an effect by triggering the operation of certain mechanisms. When individuals are presented with data suggesting an association between two variables, they routinely want additional information related to mechanisms before declaring the association to be causal in nature (Ahn et al. 1995). Social scientists are no different: they believe that causal effects are transmitted through linking processes of one kind or another.

The large social science and philosophy of science literature that has developed around the idea of a "causal mechanism" encompasses a heterogeneous set of arguments and definitions (see the suggested readings for this essay). For our purposes, we do not need to delve into the complexities of this literature. Instead, for the purposes of this essay, we understand causal mechanism to mean the intervening processes through which causes exert their effects. We propose that any relatively well-developed theory or model will provide a discussion of causal mechanisms. This is equally true for theories tested in the quantitative and qualitative research traditions: They propose ideas about the causal mechanisms that link independent variables to dependent variables.

The key issue we explore in this essay is how the qualitative and quantitative traditions *empirically assess* theories about mechanisms when making causal inferences. In the qualitative culture, researchers carry out this assessment by attempting to observe mechanisms through process tracing and the analysis of causal process observations (Collier, Brady,